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The Effects of the Value-Added Tax on Revenue and Inequality

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ABSTRACT *This paper examines the impact of the introduction of the value-added tax on inequality and government revenues using newly released macro data. We present both conventional country fixed effect regressions and instrumental variable analyses, where VAT adoption is instrumented using the previous values of neighbouring countries' VAT systems as an instrument. The results reveal – in contrast to earlier work – that the revenue consequences of the VAT have not been positive. The results indicate that income-based inequality has increased due to the VAT adoption, whereas consumption inequality has remained unaffected.*

1. Introduction

After the introduction of the personal income tax and income tax withholding, the value-added tax (VAT) stands out as one of the most important tax policy innovations.[1] The VAT has spread to a great majority of countries. The expansion of the VAT-club membership was strikingly fast in the developing world in the 1990s (see Figure 1). In many cases, the introduction of the VAT was accompanied by a reduction in customs duties and tariffs.

The benefits of the VAT can be manifold: cascading of indirect taxes is avoided, it is perhaps harder to evade than other forms of taxation, and it can easily be made compatible with international trade. In an empirical macro study by Keen and Lockwood (2010), it is found that the VAT is also a 'money machine': it has helped countries generate more revenues than they would have had without the VAT in place.

However, the suitability of the VAT for developing countries has been hotly debated. Within the theoretical work on the subject, Emran and Stiglitz (2005) argue that the VAT can be problematic when the economy has a large informal sector, whereas Keen (2008) points out that the VAT also taxes the informal sector indirectly, as the VAT is levied on some of the inputs and imports they use. Boadway and Sato (2009) offer a unifying framework, according to which the relative merits of the VAT versus trade taxes depend on a number of factors, including the ability of the government to tax firm profits. Secondly, the VAT is often seen as an inherently regressive tax. This view is especially prevalent among the representatives of non-governmental organisations. For example, a recent Oxfam report writes in its abstract: 'Tax policy in developing countries has been heavily influenced by the IMF and

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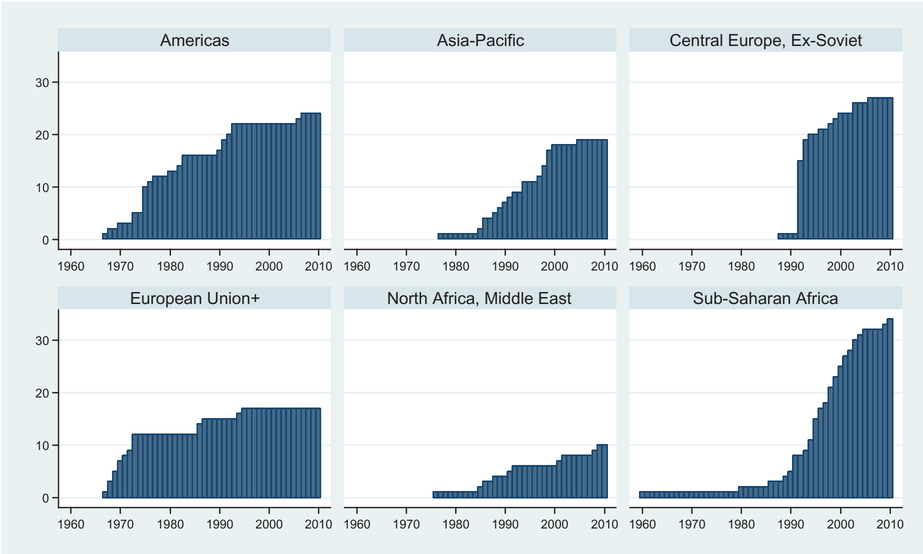

Figure 1. The adoption of the VAT by region (number of countries). *Notes:* 'European Union +' includes Norway and Switzerland. Appendix Figure S1 provides an aggregated view about the adaption of the VAT worldwide. *Source*: Authors' calculations based on IMF (2014) data.

national elites. This has had a negative impact in many cases, with a focus on indirect regressive taxation like VAT, and extensive tax incentives for companies' (Itriago, 2011, p. 1). Some academics have, however, been more sceptical, including Bird and Zolt (2005) and Gemmell and Morrissey (2005), who point out that the border taxes that the VAT has replaced could well have been more regressive.[2]

Clearly, arguments made for both points of view can be valid: a strong reliance on the VAT is relatively regressive, if the alternative is to have a well-functioning progressive income tax. On the other hand, if the VAT indeed serves as a money machine and provides the government with more revenues, these revenues can also be used for financing transfers and the provision of (public) goods that can reduce inequality. The overall impacts of the VAT on inequality are, therefore, ambiguous in theory.

The purpose of this paper is to shed light on this question, which is ultimately an empirical one, by estimating the causal impact of VAT adoption on inequality. We use newly released and high-quality data on taxation at the macro level, available from the International Centre for Tax and Development (ICTD) Government Revenue Dataset (see Prichard, Cobham, & Goodall, 2014). The data on inequality come from the World Income Inequality Database,[3] which is seen as providing a reliable database for cross-country income inequality comparisons (Jenkins, 2015). In addition to explaining the impact of VAT adoption on inequality, we also update the analysis of the revenue consequences of the VAT in Keen and Lockwood (2010), using new data (where some problematic observations are replaced), corrected specifications, and observations for more than 10 more years. While this is interesting in its own right, it is also closely linked with the inequality analysis. If it is indeed the case that the VAT leads to increased revenues, the government could use these revenues to improve public services, which would have an impact on people's wellbeing, but this improvement would not be captured in the Gini index as the value of publicly provided goods is typically not included in the Gini measures.

We present both conventional fixed effect regressions and IV estimates. The idea in the latter is that VAT adoption has proceeded in waves (see again Figure 1), and therefore we can use the neighbouring countries' earlier decisions to adopt VAT as an instrument for the VAT in the country in question. This instrument is a strong determinant of VAT adoption in the first stage, whereas it hardly has any direct impact on inequality.[4] We also run a battery of alternative specifications that test the robustness of the instrumental variable regressions. These include utilising synthetic instruments proposed by Lewbel

(2012) and checking how large a threat a situation where the instrument is also allowed to have a direct impact on the outcome – using the procedure suggested by Conley, Hansen, and Rossi (2012) – poses for the results. The main takeaway from this analysis is that the qualitative results remain robust across a wide variety of specification tests.

The paper attempts to contribute to the surprisingly small body of academic economics research examining the value-added tax. Related papers include Ebeke and Ehrhart (2011) who examine the impacts of the tax arrangement on the volatility of tax revenues in Africa, showing how a relatively large share of domestic indirect taxes have a stabilising role in tax revenue collection. Ahlerup, Baskaran, and Bigsten (2015), also for African countries, continue this work and examine the impacts of VAT adoption on revenues. They find that the presence of a VAT has not increased revenues in African countries. In turn, Lee, Kim, and Borcherding (2013), using data on OECD countries, examine the impacts of the presence of a VAT on government revenues and the size of the government, demonstrating that the government size is hardly positively affected by the VAT. Finally, using worldwide data, Ufier (2014) investigates the impact of VAT adoption on a number of outcomes using a matching approach. He finds that the presence of the VAT has led to lower inflation and government spending and increased investment and growth.[5]

More broadly speaking, our paper is related to the empirical analysis of tax systems in developing countries (for recent surveys, see Keen [2013] and Besley and Persson [2013]). Another strand of literature the paper is linked with is the cross-sectional analysis of the determinants of within-country inequality. The UNDP (2013) offers a broad overview and Hassine (2015) is an example of recent analysis, which also contains more references.

The paper proceeds as follows. Section 2 discusses the data and provides some descriptive evidence on tax policies, revenues, and inequality. Section 3 introduces the empirical methods. Section 4 presents the results on the impacts of the VAT on inequality, whereas Section 5 is devoted to the estimates of the revenue impacts of the VAT. Section 6 concludes.

2. Data and descriptive evidence

We include all countries for which data exist, except for former Soviet Union countries, where the VAT was adopted at the same time as a complete change in the economic paradigm, implying that the impacts of the VAT alone would be impossible to identify. To maintain comparability between the Keen and Lockwood's (2010) setting and ours, we keep the same country sample in the revenue estimations. The lists of countries included in the inequality and revenue estimations can be seen from Table S1 in the Supplementary Materials.

2.1. Data on measurement of inequality

The source of the data for inequality is the latest release of the World Income Inequality Database (WIID), UNU-WIDER (2015). As the WIID gives researchers the possibility to select those inequality measures that best suit the research question at hand, Jenkins (2015) concludes that the WIID can be seen as a reliable source for cross-country information on inequality. We also follow the requirement by Jenkins and report our data selection algorithm in the Appendix (Table A1).

One of the main issues in working with inequality data is that in the developed world, inequality measures are typically income based, whereas in most developing countries (apart from Latin America), Gini coefficients and other inequality measures refer to consumption inequality. Another issue is that in developing countries, household surveys are not conducted every year. For this reason, in all of the inequality analyses in this paper, we use data that are based on five-year averages. As usual, Gini values take values between 0–100.

Figure S2 in the Supplementary Materials reveals that while in some geographical areas within-country inequality has risen (notably in developing Asia), in others (notably in Latin America) it has fallen during the period we study. In Figure S3 (Supplementary Materials), the vertical line depicts the time of VAT adoption for each country. The chart suggests that, again, the picture is mixed. Inequality seems to have fallen in the time periods after the introduction of the VAT in most areas, but it has trended upwards in Asia and Latin America. This chart also suggests that there does not appear to be a clear break in the level of inequality series around the time of VAT adoption.

What kind of inequality impacts can we measure with the data available? When measuring the share of indirect taxes paid out of total income, a common finding is that indirect taxation is seen as regressive, as the share of disposable income used for consumption (the base for indirect taxation) is greater for low-income households, that is, the savings rate of high-income households is higher. We cannot take this effect into account, as the Gini index that is disposable income-based is typically calculated in the underlying data as referring to direct taxes and transfers, whereas the consumption-based Gini takes into account the burden of indirect taxes out of consumption, not income.

What we can capture by the WIID data is the tax mix: if countries, when moving to a VAT, start to have a tax system that is more dependent on flat rate indirect taxes and less dependent on progressive direct taxes, this change will be reflected as greater disposable-income inequality because of a smaller share of direct, progressive taxes. In the consumption-based Gini analysis, with sufficiently strong behavioural responses we can also detect some of the impacts of the differentiated VAT schedules. This can happen for instance if lower VAT rates on necessities are associated with a relatively larger increase in the overall demand among low-income households. While not being able to measure the differences in tax incidence that arise from different savings rates across households at different income levels is an important caveat to our analysis, one can also argue that the consumption-based inequality measures give a better picture of long-term, lifetime differences in wellbeing.

The differences in ways of measuring inequality are taken into account by us by using constant within-country definitions of inequality.[6] When we combine the series for regression analysis, we control for the type of inequality index we use (with country fixed effects). But since the impacts of the VAT on inequality can really depend on the type of measurement, we run both separate regressions for countries where inequality is measured using consumption and for countries for which inequality measures are based on income. For the latter, we choose to use the disposable income in the measurement of inequality.

2.2. Data on measurement of government revenues

Our main dataset regarding tax variables, such as overall tax revenues, is the Government Revenue Dataset, compiled by the International Centre for Taxation and Development (Prichard et al., 2014).[7] The main aim in developing the database was the need to unify the concepts used in cross-sectional data on fiscal issues, and expand the coverage of countries and tax instruments used. The main sources of the database are the IMF's Government Finance Statistics and the OECD tax statistics, as well as several regional organisations' data and IMF Article IV reports. The exact variables used in the analysis are described in Table A1 in the Appendix.

Figure S4 (Supplementary Materials) depicts the history of overall general government revenues in different regions of the world. The figure shows that developing countries have been able to raise their tax take from the 1990s to 2000 and 2010. Interestingly, the share of indirect taxation out of total government tax revenue has remained fairly stable (Figure S5 in Supplementary Materials).

Figure S6 (Supplementary Materials), which plots the revenue developments before and after VAT adoption, reveals that while total government revenues have been on the rise throughout the period, the rise in revenues was perhaps faster in some areas after the adoption of the VAT. Clearly this can also happen for other reasons than the presence of the VAT. Figures S7 and S8 (Supplementary Materials) provide similar graphs for the share of the indirect taxes out of total tax revenue. This share is, somewhat surprisingly, not consistently higher after the adoption of the VAT. The main reason for this is that the VAT has probably mainly replaced tariff revenue and import duties.

2.3. Control variables

Different sets of mechanisms can be behind the development of inequality, which guide our choice of variables that we need to control for in the analysis. First, as reflected in the ideas behind the Kuznets curve, different phases of economic development can be associated with varying levels of inequality. While our fixed effects specification accounts for permanent differences in the level of economic development, changes in GDP per capita are controlled for in the model. In a developing country context, a smaller share of agriculture is expected to reduce income disparities. It might, however, work in a different direction when a country passes a certain development phase. This is shown in Asteriou, Dimelis, and Moudatsou (2014) in an EU setting – the higher the share of agriculture, the lower the observed inequality of incomes. Population size is used to control for the size of the country.

Second, the returns to education can drive inequality (Abdullah, Doucouliagos, & Manning, 2015), and that is why we also include secondary educational attainment as one of the covariates.

Third, basic trade theory predicts that international trade can affect the distribution of income between skilled and unskilled labour; thus the need to control for openness. Recent work on the role of openness includes Wu and Hsu (2012), Kraay (2006) and Goldberg-Koujianou and Pavcnik (2007). Financial openness and development are addressed by adding a variable capturing the depth of financial market (quasi-money M2/GDP). Milanovic (2005) and Enowbi Batuo, Guidi, and Mlambo (2010) report a negative relationship between the depth of financial markets and inequality. Further, we also add a control for foreign direct investments (FDI) to capture the effect of financial globalisation. The results in Asteriou et al. (2014) suggest that a high FDI/GDP ratio is associated with higher Gini coefficients in the EU context.

Fourth, the quality of institutions and the degree of democracy can be reflected in the distributional outcomes. To take into account some of these factors, we include the index of democracy as an institutional measure from the Polity IV database to the estimations.

In the analysis of the revenue impacts of the VAT, we include the same set of controls as Keen and Lockwood (2010), which are also standard in other models explaining aggregate government revenue. These include GDP per capita, the share of agriculture of GDP, and openness. In addition, we control for the size of the country in population, demographic variables (old and young age dependency ratios) and an external pressure to increase government revenue which comes through being part of IMF crisis or non-crisis programmes. These variables are also one way to try to capture the impacts of other contemporaneous reforms that could also influence revenues and inequality. Further, a federal state dummy from Treisman (2002) is used to control for special issues which federal states (like the United States) are addressing with the adoption of the VAT.

With the exception of index variables for VAT adoption and for IMF crisis and non-crisis countries and some of the institutional variables, the data for the control variables are extracted from the World Bank's World Development Indicators Database. Data for VAT adoption and IMF crisis/non-crisis countries come from the IMF.

3. Empirical specifications

In the main equation, the dependent variable ($y_{i,t}$) is either the central government revenue (logged) or inequality, measured by the Gini coefficient. The model includes country fixed effects, α_i, and a set of control variables, $\boldsymbol{X}_{i,t}$. Given that the instrument is not randomly determined, the presence of the control variables increases the credibility of the instrumental variables estimation. Our main interest is on the coefficient, β, of the VAT adaption variable, $V_{i,t}$.[8] Therefore, the regression equation is of the form

$$y_{i,t} = \alpha_i + \beta V_{i,t} + \gamma' \boldsymbol{X}_{i,t} + \vartheta' year_t + \varepsilon_{i,t} \tag{1}$$

where $year_t$ refer to a set of year dummies and $\varepsilon_{i,t}$ is the error term. Heteroscedasticity consistent standard errors are used throughout the study. In the case of Gini estimations, year dummies refer to dummies for the five-year periods.

The equation is estimated as a fixed effects OLS model as well as a fixed effects instrumental variable (IV) model with the VAT variable being instrumented. The reason for the instrumentation is that there can be unobservable effects that influence both the dependent variable and VAT adoption. When explaining inequality, countries that become more concerned about inequality may want to introduce the VAT to finance poverty reducing expenditure. This would imply that fixed effects estimates would be biased downwards. On the other hand, if the decision-makers in the country stress the need to improve the efficiency of the tax system by moving towards a VAT, they could care less about inequality, and in this case the simple fixed effects estimates would be upwards biased. In principle, the bias can go in either direction.

As mentioned in the introduction, we use the presence of the VAT in the geographical neighbour countries as an instrument. For each country we first select neighbours as being those in the large geographical area (region such as Africa or America) and then construct two alternative ways of measuring the neighbours' impact. The first alternative instrument uses the annual share of other countries with the VAT in place in the region in the previous year ('NeighbourV'). The second alternative is to use presence of the VAT in the neighbouring countries weighed by inverse distance from country in question ('DistanceV'), also lagged by one year to avoid possible simultaneity problems.[9] Both instruments yield similar results; however, they both have different strengths. Using NeighbourV results in a stronger first stage while DistanceV contains more variation between countries of the same region in a given year.[10] As in all analysis with instrumental variables with potential heterogeneity in treatment, one must keep in mind that the estimates represent local average treatment impacts (LATE).

As the VAT has proceeded in waves, both instruments are strong predictors of the adoption of the VAT in the country in question.[11] The validity of the instruments will require that neighbours' adoption of the VAT does not have an impact on revenue raising or inequality directly. A threat to this identification would arise if the presence of the VAT in the neighbouring country were to affect the examined country circumstances directly through, for example, foreign trade. Regarding inequality, one could perhaps envisage that the VAT adoption among a region takes place at the same time that other policies (such as social programmes) are undertaken. An additional threat to identification can arise if there is time-varying spatial correlation in inequality. While some of these worries are probably of minor importance (for instance since the social protection systems in developing countries have tended to take place later than VAT adoption),[12] in the case of the DistanceV instrument we can also include region-by-year fixed effects, which will pick up all region-specific common unobservables in a given year. However, as VAT adoption has proceeded in waves, controlling for these fixed effects might capture the impacts of the policy itself, and therefore we do not include them in the reported specifications.

We also check the robustness of the results using two recent advances in instrumental variable econometrics. First, to increase the precision of the IV estimates and to examine how additional instruments could change the results, we follow the approach in Lewbel (2012) and generate additional instruments[13] from the heteroscedastic error terms. To avoid a weak instrument problem, we utilise only a subset of possible Lewbel instruments by using a subset of exogenous variables for which most significant heteroscedasticity is found in the first stage regression. Specifications also vary depending on whether controls from the 'own' country alone or also neighbours' values are used. For applications of the Lewbel approach to identification, see, for example, Emran and Shilpi (2012) and Emran and Hou (2013).

Second, we assess the sensitivity of IV estimates to violations of the exclusion restriction by using the Local-to-zero (LTZ) estimation method developed by Conley et al. (2012) for the main results in the inequality analysis. These estimates assume that the direct effect of the instrument on inequality is to be uniformly distributed between zero and some other value. The idea is that the IV result is robust if it remains qualitatively the same and still significant even in the presence of a large direct impact of the instrumental variable on the outcome.

While we are able to control for the presence of IMF-run programmes, a possibility still remains that the VAT dummy also captures the impact of some other policies that have been implemented simultaneously. Therefore, one needs to remember that the VAT dummy potentially measures the joint impact of a package of policies.

The adoption of the VAT can have had heterogeneous impacts on inequality in different countries. For this reason, we will also split the sample according to the income level. The VAT may also have different impacts that depend on the openness of the economy.[14] We therefore estimate models separately depending on the extent of openness. Furthermore, in some Gini estimations we also control for the lagged openness level of the neighbouring countries.

In the revenue estimations, we also allow for a lagged dependent variable. Hence, the regression equation is rewritten as

$$y_{i,t} = \alpha_i + \beta V_{i,t} + \gamma' \boldsymbol{X}_{i,t} + \vartheta' year_t + \lambda y_{i,t-1} + \varepsilon_{i,t} \tag{2}$$

where, to avoid the Nickell (1981) bias, the lagged dependent variable is instrumented with the fourth and the fifth lags of the dependent variable.

In some of the regression results on revenue, we follow the approach in Keen and Lockwood (2010) and also examine the interaction of the VAT with some other variables, such as the share of agriculture. When these interaction terms are added to the IV models they are also instrumented with the interaction of the control variable in question and the neighbouring countries' VAT variable.[15] When the instruments for the VAT and its interactions are exactly identified we report weak identification test statistics (Cragg-Donald Wald F stat) and underidentification test statistics (Anderson canon. corr. LM stat), whereas for the overidentified specifications the Sargan test of the over-identifying restrictions is also reported. Throughout, we also test the endogeneity of the V variable with C-tests (Baum, Schaffer, & Stillman, 2007).

In the Keen and Lockwood (2010) study, the authors also use a selection model approach. The difference between their and our study is that they run a separate adoption equation using a probit model and use it to predict a selection correction (lambda) term, which is then included in the revenue equation. The neighbouring countries' VAT adaption variable is included in the adoption equation, but it is not used as an excluded instrument as in our study. For completeness, we also report the results for the revenue equation using our IV strategy.

4. Results on inequality

In this section, we present our main results, the inequality implications of VAT adoption. Table 1 collects the estimates for the whole sample. Columns (1)–(3) contain the OLS estimates, whereas Columns (4)–(6) present the IV results with the first version of the instrument (NeighbourV). Columns (1) and (4) only contain country and time period fixed effects, Columns (2) and (5) include control variables, and Columns (3) and (6) add one control for the openness of the neighbouring countries.[16]

From the control variables, increased financial deepening (measured as M2 out of GDP) and education level are positively correlated with higher inequality. In Columns (2) and (5), increased GDP is associated with higher inequality, but the significance of this effect drops when neighbouring countries' openness is added to the model (Columns 3 and 6), which is again positively linked with inequality.

The main question is, however, what is the impact of VAT adoption on inequality. The coefficient and significance of the VAT variable differs between the fixed effects and the IV specifications. It is positive and significant in the former but insignificant in the latter. The instruments have predictive power, as confirmed by the weak identification and under-identification tests. The C-test actually does not reject the null of exogeneity of the VAT variable, but this result needs to be taken cautiously as it can be due to imprecisely estimated IV results.

Table 1. The direct effect of the VAT on inequality, period 1975–2010

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
V	2.02**	2.07**	1.81**	−0.40	2.91	−0.34
	[0.82]	[0.83]	[0.81]	[1.98]	[2.23]	[2.25]
ln(GDP per capita)		1.35*	0.96		1.50*	0.55
		[0.76]	[0.75]		[0.84]	[0.86]
Openness		1.34	1.46		1.34	1.47
		[1.73]	[1.70]		[1.74]	[1.72]
Agriculture share of GDP		−12.47*	−10.39		−11.44*	−12.84*
		[6.39]	[6.30]		[6.88]	[6.81]
Depth of financial market (M2 of GDP)		3.92***	2.65*		3.79***	2.89**
		[1.40]	[1.42]		[1.44]	[1.46]
Foreign direct investments		0.01	0.01		0.01	0.01
		[0.05]	[0.05]		[0.05]	[0.05]
Educational attainment		0.13**	0.15***		0.14**	0.14**
		[0.06]	[0.06]		[0.06]	[0.06]
Democracy index		−0.03	−0.03		−0.03	−0.03
		[0.03]	[0.03]		[0.03]	[0.03]
Neighbours' openness			13.65***			14.59***
			[3.94]			[4.09]
Number of observations	398	398	398	398	398	398
Number of countries	90	90	90	90	90	90
R-squared	0.07	0.16	0.19			
Serial correlation test stat	1.518	1.421	0.838	1.355	1.732	0.588
First stage coefficient on NeighbourV				1.17***	1.22***	1.31***
				(0.15)	(0.18)	(0.19)
Cragg-Donald Wald F stat (H_0: weak identification)				64.95	47.02	45.6
Anderson canon. corr. LM test p-value (H_0: underidentification)				0.000	0.000	0.000
C test p-value (H_0: V exogenous)				0.170	0.680	0.286

Notes: Dependent variable: Gini index. OLS models are reported in columns (1)–(3). In IV models reported in columns (4)–(6), NeighbourV is used as an instrument for V. All models include country and period fixed effects. Estimated coefficient is reported first, followed by standard error in brackets below. ***p < 0.01; **p < 0.05; *p < 0.1.

It is, however, likely that the impact of the VAT can differ between different countries and depending on the way inequality is measured. We have therefore run different sub-sample analyses, which are reported in different columns of Table 2. In addition, the different panels in the table report the results from different ways of instrumentation for the VAT variable. These include using the lagged mean value of VAT adoption in the same geographical region (NeighbourV) and the lagged inverse distance weighed VAT adoption in the region (DistanceV). Results are provided with and without controlling for the openness of the neighbouring countries. In all these cases, the model is exactly identified.

As noted in Section 3, we also report results using the Lewbel (2012) type instruments. In choosing the instruments, Breusch-Pagan tests were used to detect those variables that have sizable heterogeneity. For these models, we also report overidentification tests.

The main take-away from Table 2 is that the VAT appears to have led to an increase in inequality when inequality is measured based on disposable income. This finding is very robust across specifications, in particular, across different ways of instrumenting the VAT. Since this is a main finding where a statistically significant effect is found, we have also assessed its sensitivity to violations of the exclusion restriction by using the Local-to-zero (LTZ) estimation method developed by Conley et al. (2012). Figure S9 in the Supplementary Materials shows the effect of the VAT on income-based Gini that also allow for a *direct* impact of the instrument (NeighbourV) on inequality. These estimates assume that the direct effect of the instrument is uniformly distributed between zero and δ. The results

Table 2. Robustness checks – the effect of the VAT on inequality, period 1975–2010

		Income level sub-samples		Openness level sub-samples		Gini sub-samples	
	Full sample (1)	LIC+ MIC (2)	HIC (3)	Below median (4)	Above median (5)	Income (6)	Consumption (7)
Number of observations	398	269	129	209	189	238	193
Panel A – OLS: Basic controls	2.07**	1.71	0.87	1.24	3.14**	3.95***	0.55
	[0.83]	[1.11]	[0.84]	[1.13]	[1.25]	[1.01]	[1.48]
Panel B – OLS: Basic controls and NeighbourOPEN	1.81**	1.33	1.15	1.00	2.90**	3.70***	0.24
	[0.81]	[1.09]	[0.85]	[1.10]	[1.26]	[1.00]	[1.44]
Panel C – IV: Instrument NeighbourV,	2.91	2.37	−1.12	2.10	−1.21	10.63***	−4.09
basic controls	[2.23]	[3.30]	[2.19]	[2.64]	[5.43]	[2.36]	[6.40]
Cragg-Donald Wald F stat	47.02	24.28	16.51	34.25	7.83	51.27	7.38
Anderson canon corr LM test p-value	0.000	0.000	0.000	0.000	0.004	0.000	0.005
C test p-value	0.677	0.826	0.271	0.706	0.362	0.000	0.412
Panel D – IV: Instr. NeighbourV, basic controls and	−0.34	−2.50	0.30	−2.05	−5.38	9.20***	−12.58
NeighbourOPEN	[2.25]	[3.49]	[2.42]	[2.48]	[7.06]	[2.26]	[8.09]
Cragg-Donald Wald F stat	45.60	21.97	12.47	39.55	5.63	50.28	6.65
Anderson canon corr LM test p-value	0.000	0.000	0.000	0.000	0.014	0.000	0.008
C test p-value	0.286	0.214	0.684	0.138	0.141	0.002	0.026
Panel E – IV: Instrument DistanceV,	1.57	1.29	−5.53	1.42	−15.99	14.39***	−10.92
basic controls	[3.65]	[4.89]	[6.95]	[3.98]	[24.99]	[3.99]	[15.42]
Cragg-Donald Wald F stat	15.93	10.34	2.20	13.32	0.91	19.88	1.68
Anderson canon corr LM test p-value	0.000	0.001	0.115	0.000	0.317	0.000	0.173
C test p-value	0.887	0.926	0.197	0.961	0.172	0.000	0.333
Panel F – IV: Instr. DistanceV, basic controls,	8.32*	15.49	−5.57	1.62	8.59	17.36***	2.58
and DistanceOPEN	[4.32]	[11.02]	[7.06]	[5.10]	[29.47]	[4.39]	[21.22]
Cragg-Donald Wald F stat	13.01	3.46	2.15	8.10	0.25	20.39	0.59
Anderson canon corr LM test p-value	0.000	0.056	0.117	0.004	0.596	0.000	0.419
C test p-value	0.113	0.098	0.196	0.937	0.841	0.000	0.931
Panel G – Lewbel IV: Instr. Lewbel and NeighbourV,	2.7	2.08	−0.62	2.39	2.07	10.19***	−3.63
basic controls	[1.74]	[2.49]	[1.67]	[2.16]	[3.41]	[1.91]	[4.31]
Breusch-Pagan test for 1st st. heterosc. p-value	0.098	0.109	0.266	0.175	0.038	0.094	0.124
Number of Lewbel instruments	4	2	1	1	1	3	1
Cragg-Donald Wald F stat	15.43	14.16	13.24	24.95	8.99	18.64	7.82
Anderson canon. corr. LM test p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.001

(*continued*)

Table 2. (*Continued*)

		Income level sub-samples		Openness level sub-samples		Gini sub-samples	
	Full sample (1)	LIC+ MIC (2)	HIC (3)	Below median (4)	Above median (5)	Income (6)	Consumption (7)
Sargan test for overidentification p-value	0.968	0.919	0.430	0.651	0.200	0.697	0.871
Panel H – Lewbel IV: Instr. Lewbel and NeighbourV,	−0.23	−1.78	0.31	−1.08	−0.07	8.99***	−9.09**
basic controls and NeighbourOPEN	[1.71]	[2.55]	[1.81]	[1.97]	[3.94]	[1.83]	[4.63]
Breusch-Pagan test for 1st st. heterosc. p-value	0.059	0.109	0.264	0.040	0.052	0.254	0.140
Number of Lewbel instruments	4	2	1	1	1	4	2
Cragg-Donald Wald F stat	15.84	13.30	10.55	29.98	6.83	14.72	5.37
Anderson canon. corr. LM test p-value	0.000	0.000	0.000	0.000	0.001	0.000	0.001
Sargan test for overidentification p-value	0.743	0.859	0.997	0.160	0.097	0.797	0.632

Notes: Dependent variable is combined Gini index (gini_a) in columns (1)–(5), income-based Gini (Gini_i) in column (6) and consumption-based Gini (Gini_c) in column (7). LIC means low-income countries, MIC middle-income countries and HIC high-income countries. All estimations use five-year averaged data. In panels A-H, V is instrumented as defined in each panel. Lewbel (2012) instruments are defined as $(Z - \bar{Z})'\xi$, where ξ are the residuals from the first stage regression; Z is a vector of exogenous variables with mean values $\bar{Z}$. Here, Z variables are selected using individual Breusch-Pagan tests on the residuals to avoid weak instrument problems. The full set of control variables defined separately for each model (cf. column 5 and 6 in Table 1). All models include country and period fixed effects. Estimated coefficient on V is reported first, followed by standard error in brackets below. Cragg-Donald Wald F statistic tests for weak identification and Anderson canon. corr. LM test is for underidentification. ***p < 0.01; **p < 0.05; *p < 0.1.

show that as long as δ remains smaller than 15 in panel C (13 in panel D), the effect of the VAT on income-based Gini remains significant at the 10 per cent risk level. Because the corresponding reduced form effect of the instrument is 17.5 (and 15.8) in panel C (and D), the IV results for income-based Gini are robust to substantial deviations from perfect exogeneity.

However, Table 2 shows no impact on inequality for countries which use consumption-based inequality measurements; in some cases the impact is negative but it is not consistently significant. The difference between income- and consumption-based results must be interpreted with care, as the impact can also differ between other dimensions. In particular, high-income countries are overrepresented in the sample where Gini is income based. Additional analysis (not reported for brevity) suggests that the impact of the VAT on income-based inequality is similar in middle-income and high-income countries, suggesting that the main driving force for the result may be measuring inequality using income and not consumption rather than differences in the income levels of countries. One reason for why the impact of the VAT differs between income- and consumption-based measurement is that in consumption-based analysis, the consumers' response to the differentiated commodity tax structure can also be taken into account.

A caveat to the result is that when including region*time period fixed effects to an IV specification with inverse distance weights, the impact of VAT adoption on inequality for income-based measurement countries is not significant (results available on request). It remains significant for the fixed effect OLS model with region*time period fixed effects. While controlling for region-by-time-period fixed effects is a useful robustness check, including these additional controls also takes away some of the remaining variation. Since it is unclear how strong a concern the period-specific regional unobservable effects are, we would tend to favour the results with country and period fixed effects only.

The results do not seem to differ in a statistically significant way between income groups or depending on the degree of openness. One might also be concerned that the results using the time span until 2010 can be problematic, since most of the identifying variation in countries adopting the VAT took place earlier (see Figure 1), and the latest observations can be affected by the financial crisis. We therefore also estimate models for a shorter time span (ending in 2000). These results are reported in the Supplementary Materials Table S3. The results for the IV remain insignificant, whereas for the OLS, the sign is retained but significance drops. The positive impact of VAT adoption when inequality is measured using income remains valid for the shorter sample, as well.

5. Results on revenue consequences

We now turn to the analysis of the impacts of VAT adoption on government revenue. If having the value-added tax in place leads to an increase in overall government revenues, part of these revenues can be used in a way (as increased spending in basic public services) that also, or especially, benefits poor households. Examining the impacts of VAT adoption is also of interest, as the new ICTD Government revenue dataset is arguably an improvement in terms of comparability of time series across countries over earlier available datasets.

First, we run the Keen and Lockwood (2010) estimation equations with our updated dataset, using their preferred specifications (Keen & Lockwood, 2010, Table 2, Columns (1), (2) and (4)). The analysis is conducted for their original countries and years (until 2000; Table 3) and additional years (until 2010; reported in Supplementary Materials Table S5). However, we make small adjustments to their empirical setting, mainly adding year dummies (which Keen and Lockwood have in their model specification but are not used in the empirical part) and removing some inconsistencies from the data on population.[17] The third difference is that the data we use comes from the ICTD (as we also want to examine the longer time period which the earlier data does not cover). The implications of our updated data and other adjustments on the original results of Keen and Lockwood (2010) are discussed below.

The coefficient for V is significant and negative in two of the Keen-Lockwood type specifications (Table 3 Columns (1) and (3)), signalling that the introduction of the VAT has both a short- and long-term direct negative effect on revenue ratio.[18] In Column (2), the direct impact is not significant.

Table 3. Revenue, period 1975–2000

	KL type of models			Additional models		
	(1) OLS	(2) IV	(3) IV	(4) IV	(5) IV	(6) IV
V	−0.04**	0.00	−0.15**	0.07	0.01	0.32***
	[0.02]	[0.01]	[0.06]	[0.09]	[0.06]	[0.10]
ln(GDP per capita)*V			0.06***			−0.04
			[0.02]			[0.06]
Openness*V			0.01			−0.14***
			[0.02]			[0.05]
Agriculture share of GDP *V			0.35**			−0.80**
			[0.15]			[0.33]
Federal dummy*V			−0.01			−0.05
			[0.03]			[0.08]
ln(GDP per capita)	−0.10***	−0.11***	−0.13***	−0.10***	−0.10***	−0.05
	[0.03]	[0.03]	[0.04]	[0.04]	[0.03]	[0.04]
Openness	0.07**	0.05**	0.05**	0.07*	0.05**	0.07***
	[0.03]	[0.02]	[0.02]	[0.04]	[0.02]	[0.03]
Agriculture share of GDP	−1.54***	−0.83***	−0.95***	−1.50***	−0.82***	−0.48**
	[0.15]	[0.18]	[0.20]	[0.16]	[0.18]	[0.19]
Lagged revenue		0.57***	0.57***		0.58***	0.63***
		[0.09]	[0.09]		[0.09]	[0.09]
Dependency ratio (old)		0.64	0.06		0.55	−0.15
		[0.98]	[0.99]		[1.06]	[1.60]
Dependency ratio (young)		0.29	0.19		0.29	0.66**
		[0.28]	[0.29]		[0.28]	[0.32]
IMF crisis program		0.03***	0.03***		0.03***	0.02**
		[0.01]	[0.01]		[0.01]	[0.01]
IMF non-crisis program		0.01	0.02		0.01	0.02
		[0.01]	[0.01]		[0.01]	[0.02]
ln(population)		−0.08	−0.06		−0.08	0.00
		[0.07]	[0.07]		[0.08]	[0.09]
Number of observations	2,620	1,950	1,950	2,567	1,950	1,950
Number of countries	130	122	122	130	122	122
Serial correlation test stat	302.2	0.238	0.340	311.2	0.191	0.000
Sargan test p-value		0.542	0.492		0.525	0.463
Joint sign. of V and interactions p-value			0.094			0.022
Joint C test for exog. of end. variables p-value		0.449	0.444	0.218	0.745	0.000
Tests for weak identification						
F-test stat for lagged revenue		36.15	34.84		24.47	15.13
F-test stat for V				128.1	31.01	22.24
F-test stat for YPC*V						13.92
F-test stat for OPEN*V						61.59
F-test stat for AGR*V						40.30
F-test stat for FED*V						63.26

Notes: Dependent variable: ln(revenue). Column (1) is OLS regression, Columns (2) and (3) report IV models where lagged revenue with its own 4th and 5th lags. In Columns (4) and (5), V is instrumented with NeighbourV. In Column (6), V and all of its interaction terms are instrumented with the neighbour variables, and lagged revenue is instrumented with its lags. Standard errors are reported in brackets. All models include country and year fixed effects. F-test statistics are computed for each first stage regression separately. lnYPC is ln(the GDP per capita), OPEN is openness, AGR is agriculture share of GDP, and FED is federal dummy. ***p < 0.01; **p < 0.05; *p < 0.1.

However, Column (3) suggests that the effect of VAT is heterogeneous across countries, and one should consider the various interaction terms through which the VAT influences to obtain the direction of the effect for a country *i* with characteristics $X_{i,t}$. The interaction of the VAT with income per capita is positive and consistent with Keen and Lockwood's (2010) findings which, in turn, is aligned with the common belief that higher income countries are better equipped to deal with the administrative and compliance requirements of a VAT system. The interaction term with the federal state dummy is insignificant in both our and Keen and Lockwood's (2010) estimations.[19]

The VAT variable and its interaction terms are jointly significant at the 10 per cent level; however, the size and sign of the effect needs further analysis and cannot simply be drawn from the estimation results. The joint effect is examined below in the next sub-section.

We now turn to the results when the VAT is instrumented with the degree of VAT adoption by its neighbours (variable 'NeighbourV'). There is a possibility that the level of the revenue ratio can influence the decision of VAT adoption: on the one hand, countries more dedicated to domestic revenue mobilisation could be more willing to implement the VAT (hence leading to an upwards bias of the VAT adoption); on the other hand, it might be precisely those countries most in need of revenue that are eager to move to a VAT system. Thus, the bias could go either way, and an instrumentation strategy is implemented to deal with this bias. All the interaction terms are also instrumented by using the corresponding neighbouring values as additional instruments. Columns (4)–(6) of Table 3 present the additional IV estimation results. First stage test statistics reported at the end of the table are strong in the specifications and, in addition, Columns (5) and (6) also pass the Sargan test for instrument validity.

In Columns (4) and (5) of Table 3, the direct short-term effect of the VAT on the revenue ratio loses its significance but in Column (6) with added lagged revenue ratio, interaction terms, and control variables, the sign of the coefficient of V is positive and significant at the 1 per cent level. This could indicate that countries with low levels of revenue ratio (and perhaps with a strong need to increase revenue ratio) are more likely to implement VAT reform. Once this endogeneity is removed, the direct effect of the VAT becomes positive. However, the full effect of the VAT on revenue is a formula of direct and interaction effects which all need to be taken into consideration.

A negative and statistically significant (at the 1% level) coefficient of the interaction between VAT and openness (OPEN) is somewhat surprising and in sharp contrast with the results of Keen and Lockwood (2010).[20] They conclude that one would expect (all else equal) that the VAT functions better in more open economies since they can use their borders as tax collection points – especially in developing countries where the majority of the VAT revenue is collected at borders. Perhaps the VAT in practice in developing countries has not fully compensated for the lost revenue of tariff and custom duties' reductions.

The size of the agricultural sector has a significant and negative effect on revenue if the VAT is adopted. This is aligned with the notion that the VAT as a tax instrument has difficulties in reaching the agriculture sector.

The VAT and its interaction terms are jointly significant at the 1 per cent level but, again, the size and sign of the joint effect is analysed in more detail in the next sub-section.

5.1. The overall revenue consequences

When considering the overall effect of the VAT on revenue ratio, we follow again the Keen and Lockwood (2010) framework for calculating cumulative gains since the introduction of the VAT.

To calculate the overall gain or loss of revenues for a country that has adopted the VAT we need to sum up annual gains and losses since the introduction of the VAT. First, to calculate short-term gains we use the specifications from Column (3) and Column (6) from Table 3:

$$\Delta r_{i,t} = \hat{\beta}_V + \hat{\beta}_{YPC*V} lnYPC_{i,t} + \hat{\beta}_{OPEN*V} OPEN_{i,t} + \hat{\beta}_{AGR*V} AGR_{i,t} + \hat{\beta}_{FED*V} FED_{i,t}, \tag{3}$$

where lnYPC is a natural logarithm of the GDP per capita, OPEN is an openness index, AGR is an agriculture share of GDP, and FED is a federal dummy. Each $\hat{\beta}$ is an estimated coefficient of V or its interaction terms. From the short-term effect, we calculate the long-term effect at the end of the period by taking into account the cumulative gains through the estimated lagged revenue coefficient, $\hat{\lambda}$, as follows

Table 4. Revenue consequences, period 1975–2000

Specification: Table 3, Column (3)	ALL	AP	AS	EU	NMED	SI	AF
Countries with VAT							
Mean	0.01	0.02	−0.01	0.14	0.00	0.04	−0.05
Number with Δr > 0	37	7	9	11	2	2	6
Number with Δr < 0	36	4	12	0	2	0	18
Countries without VAT							
Mean	0.02	0.08	0.12	-	−0.01	0.04	−0.01
Number with Δr > 0	27	3	3	-	7	8	6
Number with Δr < 0	14	1	0	-	4	2	7
Specification: Table 3, Column (6)	ALL	AP	AS	EU	NMED	SI	AF
Countries with VAT							
Mean	0.04	−0.09	0.17	0.08	0.16	−0.12	−0.03
Number with Δr > 0	45	3	20	9	4	0	9
Number with Δr < 0	28	8	1	2	0	2	15
Countries without VAT							
Mean	−0.10	−0.40	−0.25	.	0.14	−0.07	−0.19
Number with Δr > 0	17	0	1	.	9	3	4
Number with Δr < 0	24	4	2	.	2	7	9

Notes: Total numbers of countries in any region may be less than actual amount of countries due to missing data. ALL = All countries, AP = Asia-Pacific, AS = Americas, EU = European Union + Norway and Switzerland. NMED = North Africa and the Middle East, and SI = Small Islands, AF = sub-Saharan Africa.
Source: Authors' own calculations.

$$\Delta r_{i,2000} + \hat{\lambda}\Delta r_{i,2000-1} + \ldots + \hat{\lambda}^{\,2000-\tau}\Delta r_{i,\tau} \tag{4}$$

where τ is the year that a country adopted the VAT.

For countries without the VAT by the end of the observation period, we simply calculate a short-term effect from an annual average of the last 10 years of the period. The long-term effect is calculated by multiplying this by $1/\left(1-\hat{\lambda}\right)$.

The results depend on the specification used and are prone to change if specifications change and they are based on the point estimates, thus not reflecting the statistical significance of the estimates. This approach, however, helps us get a more comprehensive picture of the total effect of the VAT, including the direct effect and its interactions with other variables.

Table 4 presents revenue consequences for the period 1975–2000. The results stemming from conventional fixed effects specifications (the upper panel) indicate that for approximately half of the countries that adopted the VAT the effect has been positive, and almost two-thirds of countries without the VAT would have had positive revenue gains from adopting it.

When using the IV specification (the lower panel), the cumulative gains from the VAT are still positive for more than half of the countries with VAT in place. For countries without the VAT, removing endogeneity between V and the revenue ratio reveals that countries which did not adopt a VAT by the end of the observation would have not gained but rather, on average, lost revenue in adoption. The predicted revenue gain might be negative for countries without the VAT since they have, on average, a higher trade openness index and a higher share of agricultural value added, which both combined with the existence of the VAT affect negatively the revenue ratio in this specification. Thus, even if the direct effect of the VAT is positive, the overall predicted gain (the direct impact combined with indirect interaction effects) is in many cases negative. The same observation can be seen from analyses that extend the observation period to 2010 (Supplementary Materials Table S6). Thus, we conclude that the overall revenue consequences of the VAT remain more negative than in Keen and Lockwood's (2010) original article.

6. Conclusions

Using recently released, high-quality macro data, and country fixed effect approach combined with instrumental variables, this paper examined the consequences of the introduction of the VAT on government revenue and inequality. Earlier work has demonstrated that the VAT has served well as a revenue raising device, but the VAT has also been subject to considerable criticism because of its alleged negative influences on distributional equity, especially in developing countries.

Our analysis can cover those impacts of the VAT on inequality that stem from the possible tax mix changes (if the introduction of the VAT has led to a lower reliance of progressive tax instrument) and, in cases where the inequality measurement is consumption-based, the mean effects from differentiated VAT rates. An important caveat is that with the available country-level inequality data one cannot measure those tax incidence impacts that could arise from differences in the savings rates across people with different income levels. In addition, the impacts of the VAT have been captured here with a simple indicator variable for the simple reason that information on the effectiveness and rates of the VAT are not available for such a large set of countries. We believe that despite these reservations, the importance of the VAT as a fiscal tool and the debate surrounding it justify examining its inequality impacts as well as one can.

Our results, stemming from OLS and instrumental variable regressions, suggest that on average, VAT adoption has not necessarily led to increased inequality. However, we have found fairly robust evidence[21] that when inequality is measured based on disposable income, countries with the VAT have experienced increases in inequality, whereas in countries where inequality is measured based on consumption, inequality has not increased following VAT adoption. The countries where inequality is measured using income-based Gini are more often high-income countries, and the results therefore suggest that for low-income countries, there is no evidence that the VAT would have led to widening welfare disparities. Admittedly, in the absence of an income-based measurement of inequality for these countries, the impact of the VAT on income inequality cannot be examined.

While these results suggest that the criticism levelled against the VAT is partly misplaced in the context of low-income countries, the picture is a more nuanced one. This is so because our analysis with updated data and an alternative identification strategy suggests that VAT adoption has not led to increased government revenues. This finding is in contrast with earlier work in the area.

The macro-level analysis offered by this paper should ideally be combined with careful country-level studies and country comparisons between similar countries adopting and not adopting a VAT, using more detailed incidence data. Such an analysis is likely to shed more light on the interesting country differences which the results in this paper already point to.

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Notes

1. See Ebrill, Keen, Bodin, and Summers (2001) for a broad overview of the VAT. Table S1 in the Supplementary Materials lists the years of introduction of VAT for individual countries.
2. They both survey incidence analyses of VAT and some excises and tariffs in individual developing countries. In Bird and Gendron (2007, Table 5.1), VAT is regressive in some countries and progressive in others. Unlike our paper, these studies do not examine the causal impact of VAT adoption on realised inequality.
3. UNU-WIDER, World Income Inequality Database (WIID3.0b), September 2015.
4. Lee and Gordon (2005) use a very similar instrument, the weighted averages of other countries' tax rates, in their analysis of the impacts of taxes on growth. A similar strategy is used in subsequent papers in the field of research on the macro-economic effects of tax policy, see, for example, Gemmell, Kneller, and Sanz (2014) and Liu and Feng (2015). We also discuss potential threats for the identification strategy below.
5. Ufier (2014) uses a hazard model to predict the propensity score for the matching analysis. Such an approach is not feasible in our context: for the main analysis, we use five-year averaged data, which, combined with the practice in his paper where the match is not necessarily from the same year, implies that the time series we have is not sufficient for building hazard models. Keeping the econometric approach fairly similar to that of Keen and Lockwood (2010) also has the virtue that our results are more easily comparable with that key paper in the literature.
6. We choose to use consumption-based (income-based) Gini if there are more (less) observations for consumption than income-based Gini.
7. Since we also have access to the original data used by Keen and Lockwood (2010), we check the robustness of our results to the years that data cover using the original revenue data.
8. In the case of annual data (as in our revenue regressions), the VAT variable is a simple indicator variable. When taking five-year averages for the Gini analysis, it varies between 0 and 1, depending on the number of years a country has had a VAT within a period. As very few countries have actually abandoned the VAT, those countries that have a strictly positive but smaller than unity value for V have adopted it in the later years towards the five-year period.
9. A possible concern with using spatial correlation is that OLS would be biased in the first stage (when explaining VAT adoption by neighbours' adoption). However, the use of lagged values of the neighbouring countries avoids the spatial correlation bias, see Anselin (2001).
10. LeSage (2014) recommends using sparse connectivity structures (with lots of zeros in the weight matrix), and since both instruments take into account countries only within the same continent, our approach is in line with this recommendation.
11. The first stage regression naturally includes all the same covariates that are used at the second stage.
12. They proliferated in Latin America in the late 1990s and early 2000s and in Africa still later, whereas VAT adoption took place mostly in the 1980s and 1990s (Barrientos, Niño-Zarazúa, & Maitrot, 2010).
13. We have also estimated the models using Lewbel instruments only but the effects are not precisely estimated.
14. Ebeke and Ehrhart (2011) also examine the impacts of trade; in their case on revenue stability.
15. Arguably, some of these controls could still be endogenous. However, the results survive if lagged values of the control variables are used to generate the interaction terms.
16. We also tested the importance of neighbouring countries' income levels, but it was insignificant across all specifications and was dropped.
17. For example, in their data Sudan's population stays around 0.4 million (in reality it has grown to 27 million), Sri Lanka's population stays around 50,000, and Switzerland's population growth is wrong (15 million inhabitants in the data whereas the real figure is around half of that). However, it turned out that errors in the population data did not have impacts on signs or significance of results.
18. Keen and Lockwood (2010) reported a direct positive effect of the VAT. However, adding interaction terms and controls, this effect also turns negative.
19. Also belonging to the IMF crisis country programmes is associated with a higher revenue ratio (around 3% higher), but contrary to Keen and Lockwood's (2010) results, belonging to the non-crisis IMF programmes has no significant effect on the tax ratio.
20. When extending the observation period to also cover the most recent available years of data (1975–2010), the same result emerges; see Supplementary Materials Table S5 for full results.
21. The significance of the results vanishes in one IV analysis with region by time-period fixed effects. Given that VAT adoption has proceeded in waves, controlling for these fixed effects can capture the impacts of the policy itself, and it is therefore not clear if including them is an improvement.

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Appendix

Table A1. The list of variables and their sources

	OBS	MEAN	MIN	MAX	Source
Revenue	5,389	−1.39	−7.93	0.50	Government revenue, GRD and IMF
Gini_a	1,357	37.80	18.38	74.30	Combination of income and consumption-based GINI, WIID3b
Gini_i	1,000	37.08	18.38	66.00	Income-based GINI, WIID3b
Gini_c	432	40.22	16.60	74.30	Consumption-based GINI, WIID3b
V	5,785	0.47	0.00	1.00	Adoption of VAT, IMF
NeighbourV	5,966	0.44	0.00	1.00	Adoption of VAT in the neighbouring countries in the same geographical region; lagged value, own calculations
ln(GDP per capita)	5,074	1.28	−2.11	4.10	Log of GDP per cap (constant 2000 US dollars), WDI
Openness	5,126	0.82	0.00	4.58	Imports+Exports of GDP, WDI
Neighbours' Openness (NeighbourOPEN)	5,616	0.82	0.50	1.37	Average trade openness of the neighbouring countries in the same geographical region; lagged value, own calculations
Agriculture share of GDP	4,919	0.18	0.00	0.80	Agriculture, value added (% of GDP), WDI
Federal dummy	5,616	0.15	0.00	1.00	Federal state dummy, Treisman (2002) Table 12
Dependency ratio (old)	5,468	0.06	0.00	0.25	Population 65 or older (% of total population), WDI
Dependency ratio (young)	5,468	0.35	0.12	0.55	Population 14 or younger (% of total population), WDI
IMF crisis	5,424	0.11	0.00	1.00	Dummy equal to 1 if country was a crisis IMF country, IMF
IMF non-crisis	5,424	0.12	0.00	1.00	Dummy equal to 1 if country was a non-crisis IMF country, IMF
Population	6,071	32.68	0.01	1357.38	Total Population, millions (WDI)
M2 per GDP	5,121	0.59	0.01	74.14	Financial development indicator; Financial depth: Money and quasi money (M2) per GDP, WDI
Foreign Direct investments	976	13.76	0.00	55.38	Foreign direct investments, net inflows of GDP, WDI
Education attainment	976	13.76	0.00	55.38	Percentage of total population (age 25 and over) with completed secondary education (WDI)
Democracy index	5,110	1.19	−88.00	10.00	Institutionalised democracy index (Polity IV dataset)

Notes: GRD = Government Revenue Dataset of the International Centre for Taxation and Development, WIID = World Income Inequality Database of the UNU-WIDER, WDI = World Development Indicators of the World Bank.
The series for the Gini_i is chosen by taking observations for which the variable welfaredefn in the WIID data set is either 'Monetary Income, Disposable' or 'Disposable Income' or 'Income,Net'. Gini_c is based on observations where the welfaredefn is 'consumption' or 'income/consumption' for countries in the developing world outside of Latin America. In case of duplicate observations, we keep those which are reported for the value 'all' in area, population and age coverage.